Atmospheric Physics and Chemistry

Title: Quantification and parameterization of snowflake fall speeds in the atmospheric surface-layer

The topic of this manuscript is important and timely: improving terminal velocity parameterization for snow particles by incorporating a shape factor, with potential for larger-scale simulations of drifting snow and snow distribution. Several methodological and interpretive issues remain that warrant careful clarification, justification, and additional analysis. Overall, this manuscript is suitable to be published in journal ACP before revision. The comments below are intended to help strengthen the manuscript, address potential misinterpretations, and guide revisions that will improve the manuscript's rigor.

Major Comments:

1. The manuscript relies on a cross-sectional area captured during deposition, but this may not reflect the actual area experienced by a particle while rotating in air. Please provide a more robust justification or alternative approach for estimating the effective cross-sectional area during in-flight rotation (line 165). A discussion of how particle rotation and orientation averaging affect projected area versus real cross-section during terminal settling or sensitivity analyses or a brief theoretical/empirical justification showing how deviations between captured (deposition) area and in-flight cross-sectional area would impact the calculated drag and terminal velocity would be helpful.

2. The authors compute the complexity of the snowflake from a "melting area" metric, whereas shadowgraphy data already provides shape, perimeter, and area. The author should reconsider and justify the chosen complexity metric. For example, explain the physical rationale for using a melting-area proxy for complexity. If this is a simplification, quantify its implications. Propose alternative, more directly-obtained shape descriptors (e.g., perimeter, fractal dimension, dendricity index) derived from the shadowgraphy images, and show how they correlate with the chosen metric. If feasible, re-calculate using perimeter-based or dendricity-based measures (as suggested by Yu et al., 2024, DOI: 10.5194/egusphere-2024-2458) and report how this affects the shape factor and downstream results.

3. The section of Method lacks justification for the averaging period used to characterize particle–turbulence interactions. It is better to include references and a rationale for the chosen temporal averaging window. Specifically: Cite studies showing that longer averaging periods better capture particle–turbulence interactions for comparable particle sizes and flow regimes. State the exact averaging duration used, the rationale (e.g., multiple integral time scales such as Kolmogorov time scale, Lagrangian correlation time, or several eddy turnover times), and how it compares to the turbulence dynamics in your setup. If possible, present a brief sensitivity test showing how different averaging windows influence the reported mass, size, and density distributions.

4. The manuscript asserts a first-of-its-kind measurement that (line 309), which might not accurate. It's better to rephrase to avoid overstatement and acknowledge prior work.

5. Line 216-218: what is the reason for the differences between the mass distribution?

6. Line 226: Please provide a physical explanation: higher turbulence can cause a wider spread in orientation, fragmentation, and collision rates, leading to a broader effective diameter distribution even if the mean remains similar. Include error bars or confidence intervals in Fig. 6 to reflect measurement uncertainty and sample variability. From Fig. 6(a) case 1 and case 3in Fig. 6(b), the range of effective D is almost the same. Does it mean the turbulent has little effect on the De in this situation? Discuss whether the resolved effective diameter De differs between cases, and if not, explain what the similarity implies about the influence of turbulence on mean size versus dispersion.

7. The dependence of deposition velocity vt on shape and turbulence is discussed, but potential dependencies on mesh resolution and large-eddy-scale motions are not addressed. Please explain how mesh resolution and numerical dissipation might influence vt, especially for highly irregular particles. Consider the scale separation between tiny eddies captured in the experiment and larger, real-world eddies. Discuss how larger eddies in nature could alter deposition patterns differently from your small-eddy condition. If feasible, include a brief sensitivity test showing how varying mesh density or different turbulence spectra impacts vt and deposition patterns. Clarify the practical implications for 1 km-scale simulations: to what extent can the shape-dependent vt be extrapolated, given the presence of very large-scale turbulence.

8. Lines 256-263 and Fig. 8: explain more on why do fall speeds get minimum with the highest snow density?

9. The manuscript labels two observations as surprising (Lines 237, 266-267) without strong justification. Please strengthen this part by providing quantitative evidence, theoretical rationale, or literature context that supports why these findings are unexpected. If the surprise stems from a conflict with existing models, outline how your results challenge assumptions and what future work could resolve the discrepancy.

Minor Comments:

Line 26: what is the meaning of "the motion and deposition of frozen hydrometeor settling velocity."? The settling velocity should not have the motion and deposition. Maybe better to use deposition settling velocity?

Line 32: what do you refer here the "grid-generated turbulence"?

Line 33: the author may give more explanations on this reduced effect from the literature.

Line 38: Maybe it is better to separately explain why does large particle also have better followability with the wind?

Line 46-47: it is better to directly point out the numerical definition of the “immediate range” and add the reference.

Line143：lack a short formula definition of circumscribed projected areas A and Ae.

Line 148: lack a “method” in the sentence of “Note that we estimated Vt using (Bȯhm, 1989) in this analysis. “

Line 151: determined based?

Eq（4）: how to measure the mass of each snowflake on plate?

Line 73-75: not clear. why do you think the aerodynamic density is not enough to use? Do you mean “each particle’s density and shape”? But the particle density and shape should follow a distribution function.

Line 88-89: how can you exclude the effects of tree canopy on the snow precipitation and particle motion?

Fig. 11: where does the index parameters “1.7” and “-0.32” come from?

Line 286: format error, lack a space in formula.

Line 321: “(TI) yielded 0.75 This new parameterization”, lack a “.” between sentences.

References:

Yu, Hongxiang & Lehning, Michael & Li, Guang & Walter, Benjamin & Huang, Jianping & Huang, Ning. (2024). Snow Particle Motion in Process of Cornice Formation. 10.5194/egusphere-2024-2458.